

MAYS (T. J.)

WITH THE COMPLIMENT  
OF THE AUTHOR.

all

# ADDRESS IN HYGIENE.

BY

✓  
THOMAS J. MAYS, M.D.,  
OF PHILADELPHIA.

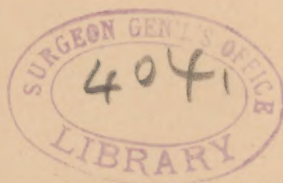


REPRINTED FROM THE TRANSACTIONS OF  
THE MEDICAL SOCIETY OF THE STATE OF PENNSYLVANIA,  
JUNE, 1890.



# ADDRESS IN HYGIENE.

BY  
✓  
THOMAS J. MAYS, M.D.,  
OF PHILADELPHIA.



---

REPRINTED FROM THE  
TRANSACTIONS OF THE MEDICAL SOCIETY OF PENNSYLVANIA,  
JUNE, 1890.

---

PHILADELPHIA:  
WM. J. DORNAN, PRINTER.  
1890.





## ADDRESS IN HYGIENE.

By THOMAS J. MAYS, M.D.,  
OF PHILADELPHIA.

---

MR. PRESIDENT: In casting a retrospect over the two years which have passed since our last meeting, I am quite certain that you will agree with me when I say that the task of reviewing in a short address all the work which has been done in the name of hygiene during this time would be a prodigious if not a profitless undertaking, and that you will pardon my confessed inability to fulfil the strict letter of the law if I confine my remarks to the consideration of a special department of this branch of medicine, and thus make an effort to compensate in concentration that which I lack in generalization.

The theme to which I shall invite your attention to-day is that of

### THE RELATION BETWEEN ARTIFICIAL INOCULATION AND PULMONARY CONSUMPTION.

The study of pulmonary consumption, like that of cholera, yellow fever, leprosy, and many other diseases, has been approached from two directions—(1) from the experimental or laboratory side, and (2) from the clinical side. It must be admitted on every hand that these are legitimate and scientific methods of investigation; and, whatever the nature of their results may be, one thing is perfectly clear, and that is, that being the products of genuine processes, they corroborate each other, and must hence be accepted. Under such circumstances one side cannot be absolutely and entirely right and the other absolutely and entirely wrong, but both must be entirely right, and must be complementary to each other.

What, then, has each method contributed to the study of the disease?

In 1865 Villemin produced tuberculosis in rabbits by inoculating them with tuberculous material. This has since been abundantly confirmed by other observers, and in 1882 Koch gave the study of tuberculosis a fresh impetus by demonstrating that a specific microörganism—the tubercle bacillus—is associated with the tubercular virus. When this organism is injected subcutaneously into the bodies of animals tuberculosis is generated. Tuberculosis may also be induced by feeding tubercle virus to animals, or by compelling them to inhale the same. It must be stated, however, that artificial tuberculosis is most readily induced by the first, and least readily by the last two methods. Rabbits and guinea-pigs are more susceptible to it than cats and dogs, and, on the whole, the disease seems more communicable when the virus is introduced into the abdominal cavity than into the eyeball.

These facts show very conclusively that tuberculosis is transmissible from man to animals through inoculation, and they have naturally given rise to the almost universal belief that pulmonary consumption is a contagious disease. The health authorities of the State of Pennsylvania and of a number of the large cities of this country and of Europe have been moved to adopt measures which have in view the suppression of this disease on the score of its contagiousness.<sup>1</sup> By common consent it must be admitted that this is a serious matter; for if this belief is correct these officers are discharging a most sacred and responsible duty, in which they should receive the encouragement of every loyal citizen. If it is erroneous it is equally clear that these officials not only perpetrate a terrible wrong on those who are afflicted with this disease, but also waste the time and energies of the people by misleading them in regard to the true nature, cause, and prevention of consumption.

<sup>1</sup> The Board of Health of the State of Pennsylvania in a recent circular (No. 26, page 6) says: "Painful as the conviction that he is liable to be a dangerous source of infection to his family and friends, as well as to the public, must be to the sufferer from phthisis, it must be forced upon him. This is the duty of his medical adviser." Further precautions given are that under no circumstances shall his expectoration be allowed to dry before it is destroyed, and that he must scrupulously avoid spitting on his handkerchief, on the floor, or on the ground, and instead must use a small spitting-flask.



Let us see, then, whether clinical medicine is able to throw any light on the truth or falsity of the contagion doctrine. In discussing this side of the question I will start out with the fundamental and self-evident proposition that—if other things are the same—those who are most exposed to a contagious disease are most liable to contract it. This may be very aptly illustrated by some of the accidents due to railway travel. While only a portion of those exposed to railroad accidents are injured or killed, it still remains true that the mortality rate from such casualties is higher among those who travel in cars than among those who do not. This principle holds true in the case of smallpox, measles, etc., and is the *experimentum crucis* in the case of consumption, if, like them, it is a contagious disease. Now, those who hold to the contagion doctrine say that the tubercle bacilli are the elements on which the contagiousness of consumption depends—*i. e.*, they are the carriers of the disease from person to person. It has been demonstrated that these germs abound in localities where the disease exists, and are absent where the disease is not found. Such localities are hospitals for consumption and the homes of those who suffer from the disease. It is inevitable, therefore, that physicians, nurses, and attendants of consumption hospitals, and intimate relatives of consumptive patients are more subject to the disease than those who are but seldom exposed. What are the facts?

Physicians who are constantly exposed to consumption are much less subject to it than are butchers, coopers, locksmiths, etc., who scarcely come in contact with it except by chance. The statistics of the Brompton Hospital for Consumption, in London, show that during a period of thirty-six years not a single clearly authenticated case of consumption arose within its walls among its twenty-nine physicians and assistant physicians, its one hundred and fifty clinical assistants, and its one hundred and one nurses, of which there existed a health record. The statistics of Friedrichshain Hospital, in Berlin, recently gathered by Dr. Fürbinger, show that during a period of sixteen years out of 459 male nurses there were 4 (2 of whom were tuberculous before entering); of 339 female nurses there were 2; of 83 physicians there were 3 (1 of whom entered with the disease) who became consumptive. Of 108 Victoria

sisters, who were engaged as nurses in the same institution from two to five and a half years, only one became consumptive.

These statistics are also strikingly confirmed by those which show the influence of the Consumption Hospital of Görbersdorf, in Germany, on the death-rate from phthisis among the inhabitants of that town. Dr. Brehmer, who had been in charge of that institution for twenty years, says that since the year 1854 more than ten thousand consumptives resided in the hospital, who daily walked the streets of the town and commingled with its inhabitants. The latter were, therefore, continuously respiring an atmosphere more or less laden with tubercle bacilli emanating from the dried expectorations of these consumptive visitors; yet, in spite of these favorable conditions for contagion, the mortality is 50 per cent. less among the Görbersdorf population since than it was before the establishment of the hospital. These figures are especially interesting in view of the assertions frequently made that the healthful influence of mountain resorts is impaired by the infectiousness of the exhalations and expectorations coming from consumptive people who go there for relief.

Then, again, it may also be said that it is not true, as is so often asserted, that the attendants of hospitals, where other diseases than consumption are treated, enjoy a similar immunity from disease. This is well shown, at least so far as typhoid fever is concerned, in the records of the Massachusetts General and the Boston City Hospitals. In the former, from 1882 to 1887, no less than seven, and probably eleven; and in the latter, from 1884 to 1888, twenty-eight cases of typhoid fever occurred among the medical attendants and employés of these institutions.

Similar negative testimony is obtained from the statistics of the contagiousness of consumption between husband and wife. Dr. Schnyder, of Switzerland, gives a record of 844 cases of consumption occurring among married people. In 445 of these the husband only, and in 367 the wife only was consumptive, while in 32 both husband and wife were affected; showing that in 812 instances there was not the least proof of contagion. Is there any reason to believe that the disease originated through contagion in the 32 cases? Dr. Schnyder



says not, for four of these cases came to him fresh from the matrimonial altar affected with the first signs of consumption, and he is of the opinion that in spite of all warnings, young people are frequently married while suffering from the disease. The late Dr. Flint gives the history of 670 cases of consumption which affected husbands and wives, and among these there were only 5 in which there was a suspicion that the disease might have been contracted from one or the other; but it is certain, he says, that the instances in which transmissibility may be suspected can also be accounted for as coincidences in a disease which is so prevalent as consumption. M. Leudet shows, too, that out of 112 widows and widowers, whose consorts died of consumption, only 7 (4 women and 3 men) became phthisical; hence there remained 105 who lived intimately with tuberculous people without contracting the disease.

About seven years ago a committee of the British Medical Association distributed circulars of inquiry among the members of the profession in regard to the liability of contagion between husband and wife or between members of the same family, etc. Ten hundred and seventy-eight answers were received. Of these, 778 were negative, 39 doubtful, and 261 were affirmative.

The large number of affirmative answers which have been received in this investigation are taken as proof of the contagiousness of this disease. This evidently is a mistake, for the aim of the inquiry was not to ascertain the number of absolutely well-demonstrated cases in which contagion was present or absent, for this would obviously have been an impossibility, but it was to collect the individual opinions of a large number of physicians as to whether they believed the disease to have been contagious in certain cases or not; and this resulted in 778 negative and 261 affirmative votes. Are we to assume, therefore, that the 261 opinions are of more weight than the 778 negative ones, and thereby imply that the former only had the fortune or misfortune of meeting cases which originated through contagion, and the latter had not? Is it not more probable that all of them witnessed cases around which hung a cloud of suspicion that they might or might not be contagious, but that 778 did not consider the proof strong enough to outweigh that which, in their minds, was in favor

of other and more powerful influences in the causation of the disease?

Now, in converging the evidence of the two sides of this question there appears to be an irreconcilable contradiction. The experimental testimony points decidedly toward contagion, while the clinical testimony just as decidedly opposes such an opinion. It must be remembered, however, that the first kind of evidence pertains only to experiments on the lower animals, and, in so far as it applies to the human body, rests entirely on a theoretical basis. It establishes the fact, however, that when the tuberculous virus is introduced under the skin tuberculosis follows. On the other hand, clinical evidence utterly fails to show that such inoculation occurs in practical life; nor does it show that those who are most exposed to the bacillus tuberculosis, as it is disseminated through the atmosphere, or through food, are more, or as much, liable to contract the disease as those who are not so exposed.

When the apparent antagonism between these two kinds of testimony is thoroughly sifted it will be found that, so far as the origination of pulmonary consumption is concerned, laboratory experiments are absolutely silent. All that they show is that the disease may be transplanted by a certain method after it has been called into existence by other causes. Glinical medicine does not, perhaps, define the exact mode of the origin of consumption, but it positively asserts that it does not arise by being transmitted from person to person through contagion. The great difficulty in the discussion of this problem has always been a neglect to distinguish between the origin and the transplantation of consumption. These two phenomena are actually treated as if they were one and the same thing, yet the original genesis of a new form of life, whether normal or abnormal, differs as much from the artificial transplantation of the same as sunlight differs from moonlight.

The truth of this is strikingly illustrated in skin-grafting. Particles of skin are planted on denuded surfaces and become thoroughly incorporated with the bodily tissues, yet such an artificial procedure gives us no knowledge of the origin and mode of genesis of the skin that is transposed. In cow-pox vaccination—another example of the same kind—a new form of life is not only transplanted to the body, but the new form



of life has a deep modifying influence on the whole organism; yet neither the operation nor its products give us the remotest idea as to the source of the virus against the action of which it protects the body. Another exemplification of this is found in the vegetable kingdom. It is well known that a graft is capable of communicating the peculiar properties of the fruit, color of leaves, etc., of the tree or plant from which it is taken, to the whole tree or plant on which it is grafted.

Dr. Darwin, after relating a number of cases in which grafting or budding of the variegated jessamine, the oleander, and the ash, infused their peculiar characteristics into the stocks which received them, states<sup>1</sup> that "many authors consider variegation as the result of disease; on this view, which, however, is doubtful, for some variegated plants are perfectly healthy and vigorous, the foregoing may be looked at as the direct result of the inoculation of a disease." Dr. Masters, in an able contribution to the subject,<sup>2</sup> says: "Cases have been observed where, from the stock *below* the graft, fruits and flowers of the same appearance as those borne on the scion have made their appearance. This has been observed in the case of the pear grafted on the mountain ash, and in other cases." In regard to the transmission of variegated leaf properties through grafting, Dr. Masters states that "a year or two since a beautiful *abutilon*, with leaves mottled with yellow, was introduced into our garden. It was very desirable that this should be propagated as largely and as speedily as possible. The scions of the variegated *abutilons* were grafted on to green-leaved stocks of other *abutilons* by many nurserymen on the continent and in this country, and it was soon found that the grafted plants produced variegated leaves from the stock. That the variegation is really due to the influence of the scion is shown by the fact that if the graft becomes separated from the stock, the leaves subsequently produced from the latter were wholly green, as before the grafting, and even the variegated leaves originally produced lost their mottled character."

Let us, then, for a moment imagine the feelings of the experimental physiologist whose mental vision of the source of

<sup>1</sup> Animals and Plants under Domestication, vol. i. p. 474.

<sup>2</sup> Grafting: its Consequences and Effects. Popular Science Review, April, 1871, p. 149.



plant-life is limited to a knowledge that grafts have the power of infusing the peculiar color of their leaves, the nature of their fruit, the odor of their flowers, and their very fibre and constitution into the stock upon which they are grafted, on being ushered into a beautiful grove of trees, or into a garden of flowers. Would he not in his ecstasy at once say to himself, it is clearly demonstrated that grafts possess the power of transmitting these properties to the plant-stock, therefore it must follow that all this diversified wealth of vegetable form and beauty is the product of inoculation by grafting? Would not his analysis be as wise and as justifiable as that of the experimental pathologist who announces that because pulmonary consumption may be occasioned by artificial inoculation, nature pursues a similar course in producing the disease? I do not by any means ascribe to myself infallibility of judgment, but I appeal to you, gentlemen, whether the mental processes employed by both the contagionists and the believer in the creative power of vegetable grafting, are not entirely identical in nature and in character?

Take away the inoculation experiments on animals and you destroy the corner-stone on which those who believe in the communication of consumption from man to man repose their belief. I am not unmindful that there are many who deny this, and who claim that their faith rests on certain and well-defined evidence of contagion. But I never yet saw a case of this kind reported which on thorough examination did not turn out to be a case of misplaced confidence, so far as positiveness is concerned. Probability exists sometimes, but this does not constitute positive proof. Nor need any one wonder at this, for it is not a question which is susceptible of decision by an appeal to specific individual instances, either for or against contagion, as has been maintained throughout this paper. But it is, as Dr. Oldendorff has recently expressed it, in a discussion of this same subject before the Verein für Innere Medicin, in Berlin, a question which must be determined by statistics; and statistics, as we have seen, give no uncertain tone in response.

When direct statistical evidence, such as that submitted in this paper, shows that among 1626 married persons, where consumption existed either in the wife or in the husband only, the same

disease occurred in the other partner in only 44 instances, the certainty of non-contagiousness is placed beyond the shadow of a doubt. Here we have the picture of more than 1600 people who were subjected to a much closer intimacy than that which obtains in any other condition of social life, occupying for years the same bed at night and the same room by day, and yet only 44, or 2.73 per cent., of the consorting partners became fellow-sufferers. Truly, if consumption is contagious, it can only be so in a very slight degree. But can the source of these forty-four cases be traced to infection? Hardly, for in a disease which is as general as consumption, it is highly probable that many of them carried within their systems a tendency to the disease, which only awoke out of its slumbering condition and asserted its power when the many varied burdens and demands of family life began to exhaust the vital resources. Again, it may be true, too, that some suffered from an active or a latent form of the disease before they were married. Indeed, Dr. Schnyder, who contributes the largest number of these cases, states distinctly that four of them came to him fresh from the matrimonial altar, victims of the disease in its incipency.

Moreover, the contagiousness of consumption is an old idea, and all the measures of prevention which are receiving serious consideration from those who believe in it at the present time were tested with disastrous results by the inhabitants of Naples more than a hundred years ago. They reasoned as follows: If consumption is contagious, then the separation of the afflicted from the well is the only logical remedy; and for sixty-six years—from 1782 to 1848—they enacted and enforced the most rigorous laws that have ever been introduced for the suppression of any disease. Every physician was fined \$180 for the first neglect to report a case under his observation, and was banished for ten years for the second offence. In every case the ceilings, walls, floors, doors, and windows of the rooms in which consumptives died were torn out and burned, and new ones were substituted. The bedding and furniture shared the same fate, and such dwellings were not inhabitable for one year. In consequence of these stringent laws the family with consumption in its midst was shunned and driven to want, and the patient was regarded as a public pest. Houses in which

consumptives died came into disrepute, and many of their owners were turned into beggars. The sick were neglected and left to die inhumanly away from their families and friends. The intolerance of this heroic treatment becomes more evident when we find that it had no influence whatever in diminishing the death-rate from this disease in Naples and in other localities where it was administered. But, however much these colossal and outrageous crimes, performed under the cloak of justice and of humanity, may shock us, they demonstrate how earnestly the Neapolitans believed in the contagiousness of consumption. If they failed to suppress it, how much can the modern contagionists expect to accomplish by requiring that consumptives should cease to spit on floors, ground, and pavements, and use fixed or portable spittoons instead. Indeed, when this advice is compared with the resolute and vigorous efforts of the unfortunate and deluded Italians, it seems more like the vaporings of a child's brain than the outcome of thoughtful and sober reflection; and it is sad to find men at this late day who are willing and anxious to repeat the superstitious follies and foibles of a century ago.

From all that has been said it seems perfectly plain to me that pulmonary consumption is not contagious in nature, and that its genesis has nothing whatever in common with that of smallpox, wound fever, and other diseases which arise through infection. The works of Villemin, of Pasteur, and of Koch have vastly enriched the science of experimental pathology, but they have absolutely no bearing on the natural origin of pulmonary consumption or on the question in dispute. No theory of any disease can be true unless it also points out the path of prevention and alleviation of that disease. Has the bacillus theory, which is so popular at the present day, rendered any such service to medical science? Has it lessened the mortality rate of this disease in the past, or does it give any assurance of doing so in the future? Can any one claim that it has not been accorded a fair and generous hearing? Was ever a medical theory launched under more favorable auspices, or received with greater enthusiasm? The medical profession, prompted by the hope, long deferred, that a knowledge of the tubercle bacillus would accomplish for phthisis what the germ idea had done for practical surgery, eagerly



and frankly accepted it, and thoroughly proved it; yet he who takes a calm and impartial retrospect of the whole situation must own that never was an *ignis fatuus* pursued which left more promises broken and greater anticipations unfulfilled than the bacillus theory, so far as it stands related to the prevention and treatment of pulmonary consumption.







